at last quite suddenly, and as if miraculously, like the fall of the walls of Jericho.

In criticism of the schedules, we may perhaps be allowed to say that personally we wish the syndicate had not followed Euclid so closely. All the practical geometry of the syllabus is mere illustration of Euclid. There are, for example, other angles than 90° easily to be drawn; arithmetical computation and experimental mensuration give new avenues to geometrical ideas, and the more avenues we can offer to pupils the better. Where the syllabus says "division of straight lines into a given number of equal parts," there appears to us too much restraint. There is no reason why a line should not be divided into many parts in any proportions, and a most educational exercise it would be. And what is the use of hiding the fact that a "preliminary" candidate cannot be prevented from having a good working knowledge of Book vi., although it is wise enough to keep the demonstrations to a later stage? Any boy understands that maps may be drawn to different scales, and this is almost the whole of the sixth book of Euclid. As for construction of tangents to a circle and "construction of common tangents to two circles," we would let a student draw these without introducing any idea of difficulty and we would ask him, by dropping perpendiculars on tangents from centres, to find the real points of contact. As soon as a boy can draw a right-angled triangle, measuring the sides and using arithmetic to find sines, cosines and tangents, he ought to begin trigonometry. If he knows the mere definition of $\tan A$, he ought at once, by merely exercising his common sense, to be able to draw the angle the tangent of which is given. A common-sense knowledge of right-angled triangles is really a knowledge of solution of triangles in general. But until the artificial bulkheads between the various water-tight compartments of mathematics are swept away, we suppose that it will not be possible to give to very young schoolboys the power to solve trigonometrical problems. If the syndicate would condescend to study the elementary syllabuses of Science Subjects I. and V_p of the Education Department, we think these courses of studies might become much easier and much more valuable.

But is not ingratitude the meanest of sins? And may it not show wisdom in the syndicate that it avoids changes which may seem to be too sudden and too great? Besides, it is to be recollected that almost every candidate who has followed this course has also taken a course in experimental science, into which weighing and measuring, the uses of squared paper and logarithms, and the ideas of the calculus have entered in all sorts of common-sense ways. Even taken by themselves, the schedules mark a great step in our experiment of finding a method of teaching mathematics suitable for boys of the Anglo-Saxon race. A beginning has been made in disenchanting the English school system of those pedagogic dogmas which have tied teachers and pupils hand and foot. Teachers and examiners will ask for more and more freedom as they find that it is altogether good. Hitherto, the average English boy has believed himself to be stupid because he was unable to reason about things unknown to him; hitherto, the average English teacher of mathematics has thought of himself as a dull, tired usher because he has had no interest in teaching; in future, pupils and teachers will feel with complacent pride that they have come to their inheritance as thinking, useful human beings. We look forward to very great results, and we are not going to give credit in particular to any one of the ten or twenty names that rise before us of the men who have helped to make this reform. Those who are dead had their reward in knowing that they helped towards a reform that was certain to come; those who are alive have the reward of knowing that they were commissioned to keep alight the torches lit by their much-loved predecessors.

With the exception of the Society of Arts, no institution of the country has been so successful in initiating scientific reform as the British Association. A Committee was appointed in 1874 (the present writer is proud to think he was a member of it) for improving science teaching in schools, and another for improving mathematical teaching, and although the members of these Committees were mostly men of influence, their efforts led to no important results for many years. But ten years afterwards, the report of a British Association Committee on the teaching of science acted on the scholastic world like the prince's kiss in the story of the Sleeping Beauty, and in 1901 the British Association proceedings in the new Education Section acted in much the same magical way in relation to the teaching of mathematics. Many mathematical masters were feeling hopeless about reform, but without jealousy, with great enthusiasm, with the most wonderful forgetfulness of differences in small matters, they joined together to assist the British Association Committee of Mathematicians. There can be no doubt that this evidence of a desire for reform among the schoolmasters had a great effect upon the members of the Committee who were not in immediate touch with the schools. All the tact, patience and resourcefulness of a chairman eminent for these qualities might have been unavailing in dealing with a Committee the members of which were all men of great individuality had it not been for the schoolmasters' memorial. Anyone who knows the history of this reform must recognise its peculiarly English characteristics—the conservative clinging to past methods because of the recognisable good in them, even among the most radical reformers; the efforts of individuals in low and high positions gradually making converts in spite of the seeming hopelessness of reform; the unwillingness of men in high positions to lend their names to the movement, the virtue of which they were aware of, so long as they thought that only unrest and disturbance could accompany it; and their concerted action as soon as it was evident that a great reform was possible. And now, because it has occurred in the English way, we know that the reform is real, that it will have a fair chance, that it will go on year after year for many a year to come. This is no case of a thin end of a wedge, for no force is really required. It would be bad policy to make too great a change at once. Freedom has been given to teachers, a freedom much sighed for, a freedom which will create enthusiasm. Those who are most determined to make the reform complete are most anxious to proceed cautiously and to smother intemperate zeal.

JOHN PERRY.

THE THEORY OF THE GAS MANTLE.

A NUMBER of papers have been recently published which deal, either directly or indirectly, with the cause of the high efficiency of the incandescent gas mantle.[1] Space does not permit us to enter at all fully into the details of these papers, but it is of interest to consider some of the questions which they raise.

The high luminosity of the mantle and its still more remarkable dependence on a particular composition have long been recognised as facts calling for some special explanation, and many have been the hypotheses advanced to account for them. The simplest of these is that which

[1] "Zur Theorie des Auerlichtes," by W. Nernst and E. Bose (*Physikalische Zeitschrift*, 1900, i. 289).
"Theory of the Incandescent Mantle," by A. H. White, H. Russell and A. F. Traver (*Journal Gas Lighting*), lxxvii. p. 879, and lxxix. p. 892).
"Theory of the Incandescent Mantle," by A. H. White and A. F. Traver (*Journ. Soc. Chem. Industry*, 1902, xxi. p. 1012).
"The Conditions Determinative of Chemical Change and of Electrical Conduction in Gases and on the Phenomena of Luminosity," by Prof. H. E. Armstrong, F.R.S. (*The Chemical News*, May 23 and 30, 1902).
"The History of the Invention of Incandescent Gas Lighting," by Auer von Welsbach (*The Chemical News*, May 30, 1902, p. 254).

regards the mantle's luminosity as an ordinary high temperature effect; as showing how the phenomena are accounted for by this explanation, we may quote the view put forward by Mr. J. Swinburne (*Journal* of the Inst. Elect. Eng., vol. xxvii. p. 161). Mr. Swinburne will have nothing to do with selective emissivity, but states that "all bodies" (presumably solid bodies) "at the same temperature give out light of the same colour." The Bunsen flame, he argues, in which the mantle is immersed, is extremely hot, and the mantle's luminosity is due to its very nearly attaining this temperature. A bad radiator (such as thoria) will reach the same temperature as the flame, but as it radiates so little energy will give but little light; what light it does give, however, will be of high luminous efficiency. A good radiator (such as ceria) will radiate energy so fast that it will not attain anything like the flame's temperature. It is, therefore, only necessary to add sufficient ceria to the thoria to increase the emissivity enough to get a good quantity of radiated energy, but not enough to lower the temperature unduly, in order to get a composition giving a brilliantly luminous mantle. This explanation does not appear to us sufficient, especially when one considers that it is polished, and not white, bodies which are bad radiators, so that if it is legitimate to argue from their behaviour at low temperature, thoria would be expected to be but little inferior as a radiator to ceria or even carbon. Also there seems some reason to think that selective emission is more probably the rule than the exception (see, for example, the work of Nichols and Blaker, published in the *Physical Review*).

Le Châtelier and also Nernst (*loc. cit.*), arrive at the same final result as Mr. Swinburne—namely, that the mantle is so bright because it more nearly approaches the temperature of the flame than any other body similarly placed—but by a different argument. The experiments which they made led them to conclude that the emissivity of the mantle is poor in the region of the red rays; hence there is little energy lost in non-luminous radiations, and the mantle can in consequence come up to the high temperature of the flame, at which it begins to radiate well, especially in the region from the green to the violet. The selective emissivity of the mantle material has therefore a double effect; it increases the luminosity at a given (high) temperature, and it enables the mantle to attain a higher temperature than a black body, because the total loss of energy by radiation is diminished. Bunte, on the other hand, claims that the assumption of selective emissivity is unnecessary, and that the mantle is at a higher temperature than the flame (*Berichte* Deut. Chem. Ges., 1898, i. 5). This view is supported by experiments he performed, in which different substances were raised to incandescence in pairs in the inside of an electrically heated tube; no appreciable difference could be observed in the light given by carbon, thoria, ceria or the material of the mantles. It remains to be explained how the temperature of the mantle can be higher than that of the flame. This is due, he and Killing suggest, to the catalytic action of the ceria, which, by oscillating between a low and high state of oxidation, increases the rate of combustion at the mantle surface and so raises its temperature. The thoria is necessary, according to Killing, to give a large surface over which the ceria molecules are spread; and Bunte suggests that it also acts as an insulator between the ceria molecules, enabling them to maintain the high temperature that their catalytic action produces.

Obviously, the simplest method of testing the accuracy of some of these different hypotheses is to measure the temperatures of mantles of different composition. An attempt to do this has been made quite recently by Messrs. White, Russell and Traver (*loc. cit.*). The temperatures were measured by means of small thermocouples, and (by making measurements with couples of different sizes and so obtaining data for extrapolation) they claim to have arrived at a method giving with considerable certainty the temperatures of flame and mantle. Even if the accuracy of the absolute values thus obtained be impugned, the relative results are not so subject to the same objections. These experimenters find that the temperature of the mantles and flame is from 1500° C. to 1700° C.; that the mantle is at a slightly lower temperature than the flame and at very nearly the same temperature whatever its composition; and, especially, that a pure thoria mantle is at a slightly higher temperature than one of thoria and ceria. Some actual results illustrating these points may be quoted from their paper:—

Composition of Mantle. Per cent	Temperature of Mantle. C.	Temperature of flame. C.	Candle-power per sq. in.
100 thoria	1560°	1630°	3·8
99·5 thoria & 0·5 ceria	1520°	1630°	34·0

The mantles used are said to have been identical in every respect except in their chemical composition. The differences in temperature are not very great, but, such as they are, they do not harmonise with the theory of le Châtelier and Nernst, since they show the thoria mantle to be the hotter; at the same time, they support this theory as against that of Bunte by showing the mantle to be at a lower temperature than the flame. The results also support the views of Mr. Swinburne, which require that the order of the temperature should be the same as that observed. In some other experiments, the results were less conclusive, the illumination varying from 2·5 to 48 candles with practically no temperature difference. Mantles with a high percentage of ceria were not tested. The authors themselves conclude that the illumination is to a greater degree a specific function of the material than it is of the temperature, and that the particular thoria-ceria mixture is a solid solution capable of transforming the heat of the flame into light more economically than any other substance yet known.

If this explanation is to be accepted, the mechanism by which this transformation is effected remains to be explained. In that part of the paper by Prof. H. E. Armstrong (*loc. cit.*) which deals with the question of luminosity, we find a suggestion as to what this mechanism is. Prof. Armstrong's paper is of a comprehensive and far-reaching character, dealing with many things besides luminosity in general and that of the mantle in particular, but it is only its bearing on these questions that we can consider here. Prof. Armstrong thinks that "luminosity and line-spectra are the expressions—the visible signs—of the changes attending the formation of molecules from their atoms, or, speaking generally, *that they are consequences of chemical changes.*" Applying this to the Welsbach mantle, after referring to Bunte's hypothesis, he says, "this undoubtedly must be the case; but I would go further, and regard *the chemical changes* occurring at the surface as the direct seat, or origin as it were, of the luminosity. Probably a higher oxide is alternately decomposed and reformed—in other words, *the process is one of oscillatory or recurrent oxidation.*" This process, then, gives direct birth to the luminous radiations and accounts for the high efficiency of incandescent oxides generally, such as the lime and zirconia light and the Nernst glower. A somewhat similar conclusion is arrived at by Dr. Auer von Welsbach (*loc. cit.*), who considers that the ceria when in one or other state of oxidation can form a compound with the thoria: hence "if reduction takes place, there is also decomposition, and if oxidation, there is recombination of these elements; these reactions may go on several million times a second, and molecular shocks are produced which give rise to luminous oscillations of the ether, and

the body becomes incandescent." Both Prof. Armstrong and Dr. Welsbach attribute the importance of the special composition of the mantle to this particular mixture forming a solid solution of a dilution favourable to the occurrence of the oscillatory changes.

We have endeavoured to put forward a summary, of necessity brief, of some of the principal theories which have been advanced to account for the luminosity of the mantle. Although it is true that some of these theories, if regarded as individually sufficient to account for the phenomena, lead to conclusions mutually inconsistent, yet there is no reason why they should not all contain some part of the truth, unless the experiments of Messrs. White, Russell and Traver be considered as sufficiently conclusive against the idea of the mantle being hotter than the flame. Such a result does not preclude the possibility of catalytic action, for the additional energy thereby developed may be all dissipated in luminous radiations. It seems that the most satisfactory explanation that the present experimental data justify is that the high luminosity is due to a combination of the good radiating power, the high temperature and the selective emissivity of the mantle. The first accounts for the high candle-power at the temperature attained; the second, which is due partly to the selective emissivity diminishing the useless radiation losses and partly, no doubt, to the catalytic action of the ceria molecules, is responsible for the high luminous efficiency of the light, so far as this is a function of the temperature; whilst the third, most probably due to the recurrent chemical changes, accounts for the high luminous efficiency so far as it is a function of the material. Thus all these causes, operating together and assisting one another, combine to produce one of the most efficient artificial illuminants that the ingenuity of man has devised.

MAURICE SOLOMON.

THE EXPLANATION OF A REMARKABLE CASE OF GEOGRAPHICAL DISTRIBUTION AMONG FISHES.

MOST text-books and papers discussing geographical distribution have made much of the range of a genus of small fishes, somewhat resembling trout, the Galaxias, commonly described as true fresh-water forms, which have long been known from the extreme south of South America, New Zealand, Tasmania and Southern Australia. The discovery, within the last few years, of a species of the same genus in fresh water near Cape Town, whence it had previously been described as a loach by F. de Castelnau, has added to the interest, and has been adduced as a further argument in support of the former existence of an Antarctic continent. In alluding to this discovery when discussing the distribution of African fresh-water fishes in the introduction to my work "Les Poissons du Bassin du Congo," in 1901, I observed that, contrary to the prevailing notion, all species of Galaxias are not confined to fresh water and that the fact of some living both in the sea and in rivers suffices to explain the curious distribution of the genus; pointing out that in all probability these fishes were formerly more widely distributed in the seas south of the tropic of Capricorn and that certain species, adapting themselves entirely to fresh-water life, have become localised at the distant points where they are now known to exist. Although as recently as October last the distinguished American ichthyologist D. S. Jordan wrote (*Science*, xiv. p. 20) "We know nothing of the power of Galaxias to survive submergence in salt water, if carried in a marine current," it is an established fact, ascertained some years ago by F. E. Clarke in New Zealand and by R. Vallentin in the Falkland Islands, that *Galaxias attenuatus* lives also in the sea. In New Zealand, it periodically descends to the sea, where it spawns, from January to March, and returns from March to May. In accordance with these marine habits, this species has a much wider range than any of the others, being known from Chili, Patagonia, Tierra del Fuego, the Falkland Islands, New Zealand, Tasmania and Southern Australia.

I now wish to draw attention to a communication made by Captain F. W. Hutton in the last number of the *Transactions* of the New Zealand Institute (xxxiv. p. 198), "On a Marine Galaxias from the Auckland Islands." This fish, named *Galaxias bollansi*, was taken out of the mouth of a specimen of *Merganser australis* during the collecting excursion to the southern islands of New Zealand made in January, 1901, by His Excellency the Earl of Ranfurly.

It is hoped that by giving greater publicity to these discoveries, the family Galaxiidæ will no longer be included among those strictly confined to fresh waters and that students of the geographical distribution of animals will be furnished with a clue to a problem that has so often been discussed on insufficient data. As observed by Jordan (*l.c.*), "all anomalies in distribution cease to be such when the facts necessary to understand them are at our hand."

Of the fresh-water species of Galaxias, eight are known from New Zealand and the neighbouring islands, seven from New South Wales, three or four from South Australia, one from West Australia, two from Tasmania, seven from South America, from Chili southwards, and one from the Cape of Good Hope.

G. A. BOULENGER.

LOCAL MAGNETIC FOCUS IN HEBRIDES.

IN the course of a recent survey in the Hebrides, Captain A. Mostyn Field, in H.M.S. *Research*, found and examined an area in the entrance of East

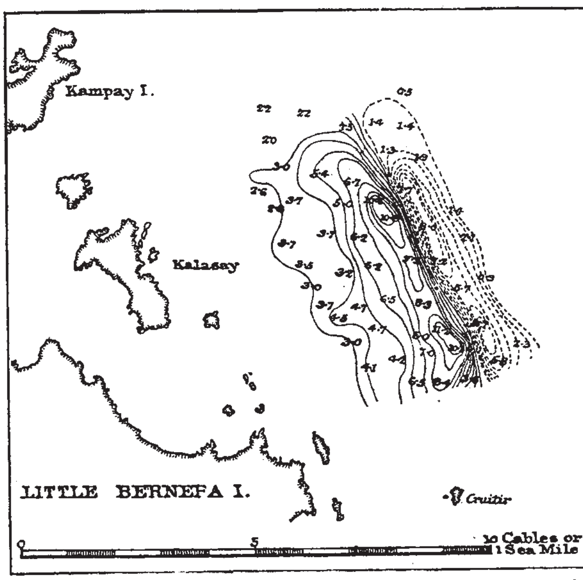

FIG. 1.—Examination in 1902 by H.M.S. *Research* of an area of magnetic disturbance in East Loch Roag, Lewis.
Lines of equal disturbance westerly from the normal declination shown in continuous line.
Lines of equal disturbance easterly from the normal declination shown in broken line.
Normal declination 22° W.
The figures express degrees and decimal parts.
Depth of water over area from 15 to 17 fathoms.

Loch Roag, Lewis, where there is considerable local magnetic disturbance. A plan showing the deviation from the normal declination of the compass needle at